

which surround us are due to the interaction of something material and something spiritual, or (to express it otherwise) to the fact that something spiritual uses the material as its instrument or organ. This seems to imply a dualism, but he also holds it possible that "there may be some intimate and necessary connection between a generalised form of matter and some lofty variety of mind."

The arrangement of the various topics is not always the best possible. This is partly caused by the inclusion of reprints from well-known journals—a practice which is open to criticism. But apart from these slight defects the book deserves hearty commendation.

*The Fox.* By T. F. Dale. (Fur, Feather, and Fin Series.) Pp. xiii + 238; illustrated. (London: Longmans, Green and Co., 1906.) Price 5s.

"THE fox," writes the author in his opening paragraph, "is at home in Europe, Asia, including India, a great part of Africa, the whole of North America, and a distinct but allied species, *Canis virginianus*—known as the grey fox in the United States—is found in South America." If he had tried to compress as many errors as possible into a single sentence, he could scarcely have succeeded better. The fox is unknown in India proper, it inhabits only the northern fringe of Africa, and the grey fox (*Urocyon cinereo-argenteus*) is a native of North and not of South America. This is one of those numerous instances where authors of works on popular natural history will go out of their way to refer to subjects which they do not understand, and which do not concern them. Had Mr. Dale kept within his proper limits, we should have had nothing but commendation to bestow upon his work, in which the fox is discussed from the point of view of the sportsman and the farmer in a very thorough manner. The eight illustrations by Messrs. Thorburn and Giles are all that can be desired, although one of them follows somewhat closely on the lines of a well-known sketch by the late Mr. Wolf.

R. L.

*Oologia universalis palæarctica.* By Georg Kause. Part i. (Stuttgart: Fritz Lehmann, Verlag; London: Williams and Norgate.)

THIS is the first part of a beautiful egg book, printed entirely on separate sheets of cardboard, two sheets being devoted to each species—one of coloured figures of the eggs, the other of letterpress, backed with references to the specimens figured. The text is in German and English, and comprises a large number of synonyms and local names, and a short description of the range of the bird, its breeding habits, nest, eggs, &c. The four species treated of in the first part are the golden eagle, quail, song thrush, and raven, as many as sixteen (odd) eggs of the last-named bird (from different localities) being figured. In the case of the song thrush we have five "clutches," and in that of the golden eagle a clutch of two eggs and three single ones. The colour printing has been very successful, and admirers of eggs will welcome the excellent selection of varieties which has been figured, of each of which the "data" are given. We cannot extend the same praise to the English version of the letterpress, which is crude, too literal, and disfigured by unfamiliar words and expressions. However, it is possible to understand what is meant, although the remark on the quail that "the ♀ only breeds, the male is polygamons," reads strangely until we substitute broods for breeds and correct the misprint.

The work is to be complete in 150 parts, and Messrs. Williams and Norgate point out that on the publication of Part ii. the price per part will be raised from fifteen to eighteen pence.

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## LETTERS TO THE EDITOR.

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### Osmotic Pressure.

IN the concluding sentence of his most interesting letter on this subject in your issue of May 17 (p. 54) Mr. Whetham states that "The theory of ionic dissociation rests upon electrical evidence, and by such evidence it must be tried." It is unnecessary to dwell on the importance of the pronouncement.

Will Mr. Whetham kindly tell us *how we know* all the things which—in the final paragraph of his letter—he so confidently asserts that we know; in fact, what *precisely* the electrical evidence is upon which the theory of ionic dissociation *now rests*. He is a recognised master of lucid exposition and will be able, I am sure, as counsel of the whilom advocates of the doctrine of molecular suicide in solution, to state the case fully and fairly on their behalf. When we have this statement it will perhaps be possible to consider the validity of his modest contention and whether electricians alone have the right to pronounce judgment. A plaintiff is usually sure of his case before his cross-examination takes place.

This request is preferred in no adverse (i)r(onal) spirit, simply because I feel that it really is necessary that we should be informed where we are exactly. Our friends the ionic dissociationists are incorrigible squatters and seem to think that they have acquired the right of preemption over their adversaries' property; it is difficult to know, as they object to stock-taking, whether they have given anything in exchange for that they have lifted and what they have jettisoned of their original property; and until the electricians' title-deeds are shown and submitted to careful scrutiny, chemists can scarcely be expected to admit that they are ousted from possession.

As a chemist and a friend of the poor molecules, I feel that the aspersion of immorality should not be allowed to rest upon them for ever unless the evidence be really condemnatory beyond question. In any case, it is important that we should discover the true nature of the crime committed in solution; to cloak the inquiry by restricting it to thermodynamic reasoning—a favourite manœuvre of the mathematically minded—is akin to using court influence in abrogation of full and complete investigation; such a course may satisfy the physicist but is repulsive to the chemist, who, although able, perhaps, to imagine the existence of a frictionless piston, yet desires, in the first place, to get nearer to a knowledge of what happens to the real tangible piston of practice. HENRY E. ARMSTRONG.

MR. WHETHAM'S letter in NATURE of May 17 (p. 54) raises clearly the whole question of the applicability of thermodynamic reasoning to osmotic phenomena. As my views as to the value of thermodynamic reasoning appear to be somewhat heterodox, may I indicate some criticisms of his remarks?

All thermodynamic proofs assume the truth of the "second law." Now the machinations of Maxwell's demon have shown clearly that the meaning of this law, when interpreted in terms of the molecular theory, is merely that, in the processes considered, no differential treatment is applied to the molecules in virtue of their different velocities. The law may or may not be true in any particular case. It cannot be said that there is any *a priori* support for it, or that a proof of its validity for one small branch of phenomena would justify its application to a totally different branch.

In all treatises with which I am acquainted, when the law has been stated, the only reasons alleged for believing it to be true are those derived from our inability to construct a heat engine which will work without equalising temperature. A few pages, before or after, will be found the statement that we cannot construct a reversible heat engine; but it is not pointed out that the irreversibility of all actual engines would mask the effect of a violation of the second law, unless that violation were very complete and the separ-

ation of the molecules into high and low velocity groups very nearly perfect. A demon might be slaving with the most commendable energy, but all his exertions would be rendered inoperative by the imperfections of our apparatus. To my mind, the evidence for the second law, even applied to the best actual heat engines, is extremely slight.

But even if the evidence were overwhelming, there would be no justification for applying the law to a process of such an entirely different nature as osmosis, where, moreover, there is some presumption that it is not true. No actual membrane is perfectly semi-permeable; some molecules of the solute pass through; it is not wildly improbable that these molecules possess velocities within some narrow range. But if this is so, Maxwell's demon is at work, the second law is not applicable, and thermodynamic reasoning is absurd. Definite experimental proof must be offered before the validity of the law for osmosis can be considered even probable. Some progress might be made by examining the same membrane at different temperatures; if its "degree of imperfection" did not vary rapidly with the temperature, the existence of such a separation as has been suggested would be rendered less probable.

Mr. Whetham has offered some proof already. He points out that there are five assumptions involved, and asserts that the truth of all of them is proved by the agreement between theory and experiment. But he ignores the possibility that two or more of the assumptions may be incorrect and that the errors thus introduced may cancel each other. He offers a particular solution of an equation containing five variables, and assumes that it is the only solution possible.

It must be remembered that there is not perfect agreement between theory and experiment. The errors are larger than those involved in the direct measurement of the pressure and the other quantities involved; there is a systematic error. But this is due, say the thermodynamicists, to the imperfection of the membrane. Exactly so; but that imperfection may invalidate the whole proof; in order to support their proof they may be denying one of their fundamental assumptions.

Mr. Whetham says that to reject the theory because there is no perfect membrane would be as absurd as to reject all thermodynamics because there is no reversible engine. I agree; but then I am such a heretic that I reject both. Our inability to construct a perfectly reversible engine is connected with the impossibility of handling individual molecules; friction and the rest would vanish if we could replace the material cylinder by a swarm of trained demons. When we have constructed a perfectly reversible engine we shall be possessed of the powers of those demons, and we shall be no longer bound by the second law, which merely asserts that we do not possess those powers. So far as physicists are concerned, reversible thermodynamics is "a vain thing."

Neither am I convinced of the perfection of Mr. Whetham's two perfect membranes. They are doubtless perfect so far as the solute is concerned, but his assumption (2) may be violated by the molecules of the solvent. It is quite possible that it is the swifter molecules which escape in the vapour and the slower which escape into the solid, and that, if our experimental devices were sufficiently delicate, we could use the separation thus effected to perform useful work. At any rate, proof is required to the contrary, before thermodynamic deductions can be made with accuracy.

So far as I can see, thermodynamic reasoning applied to osmotic phenomena, as to most others, proves nothing but that the sum of the errors introduced by the various rather doubtful assumptions is not very different from zero—a result that does not seem to me worth the labour that has been expended in obtaining it.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, May 20.

#### The Oscillation of Flame Cones.

PROF. GALLOWAY (*NATURE*, April 19, p. 584) considers that my explanation of the phenomenon described by Mr. Temple in his letter (March 29, p. 512) is inadequate, and he offers a different explanation. With the view of deciding the question some experiments have been made here by

Mr. C. E. Whiteley. I may perhaps repeat that the phenomenon in question is the continued descent and re-ascent of the inner cone of a coal-gas and air flame when a suitable mixture of the two is ignited at the end of a glass tube fixed so as to form a prolongation of the metal tube of a Bunsen burner.

The following results were obtained by Mr. Whiteley:—(1) The continued oscillation of the inner cone could not be established with a forced supply of both gas and air, but only when the air was sucked in by the injector action of a gas jet, as in the ordinary Bunsen burner. (Mr. Temple informs me that this was also his method of working.) (2) The continued oscillation of the inner cone could be maintained when the apparatus was tilted even to horizontality or beyond. (3) When the inner cone began to descend a back pressure was immediately produced in the ascending current of gas and air.

I think the determining influence is clear from these observations. When the cone begins to descend and causes a back pressure this will momentarily check the indraught of air without materially checking the supply of gas. A stratum of mixture containing less air is thus produced; its rate of inflammation is less than its upward velocity, and so the cone is carried to the top of the tube. Soon the normal air supply is re-established, a mixture with a higher rate of inflammation is restored, and the cone again descends.

A confirmation of this explanation is afforded by two further observations:—(4) a shortening of the glass tube increases the rapidity of oscillation in conformity with the shorter distance to be traversed by the altered stratum; (5) a "capacity" in the form of a globe at the bottom of the glass tube stops the oscillation. Such an arrangement would both damp the back-pressure impulse and obliterate stratification.

Observations (2) and (5) show, I think, that the chimney-like action suggested by Prof. Galloway cannot be the determining cause, and indeed this could hardly be expected, inasmuch as such action would increase the aspiration of air and produce a mixture having a higher rate of inflammation, a condition which would oppose the other effect, viz. the increased upward velocity of the mixture to which alone Prof. Galloway alludes.

My own previous explanation was inadequate to explain the continued oscillation, and only important in relation to the lighting back of Bunsen flames.

ARTHUR SMITHELLS.

The University, Leeds, May 19.

#### Ancient Fire Festivals.

IN reference to your series of articles which have recently appeared in *NATURE* on Stonehenge and the ancient festivals, I send you the following notes on a Wiltshire celebration of the August fire customs. Tan Hill Fair is held on August 6, and the coincidence of the name Tan (Celtic for fire) and the date point to a time long prior to our era, when the fire festivals were annually held.

This fair, the origin of which is lost in antiquity, is held in the very last place likely to be chosen for such a purpose, and must have had its beginning at a time when men assembled there for some purpose very different to what brings them there now, for neither roads nor waterways (conditions essential to most fairs) lead to Tan Hill.

Tan Hill is on the highest part of the downs (near Devizes, north Wiltshire), 958 feet above sea-level, looking down on Avebury and dominating the whole country, and crossed only by British trackways which lead to the fair.

Sacred fires lit of old on this Tan Hill would have been seen from Martinsell (near Marlborough), Hackpen, Oldbury, and for miles around, and were probably eagerly watched for by the people taught to expect the blessing on the crops of the ensuing year consequent on these fires; and it is on this bleak, desolate down that one of the largest fairs of the country is held.

Fairs in Ireland and in Wales carry on the same tradition of the ancient fire festival held in August, as well as this one at Tan Hill.

In ancient Ireland this August celebration was called "the *Lugnassad*," the feast of Lug (a sun god), and according to Prof. Rhys "this festival was the great event